MOST IMPORTANT ERRORS

IN

CHEMISTRY,

ELECTRICITY, AND MAGNETISM,

POINTED OUT AND REFUTED;

AND THE

PHENOMENA OF ELECTRICITY

AND THE

POLARITY OF THE MAGNETIC NEEDLE

ACCOUNTED FOR AND EXPLAINED.

BY

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"Naturam expellas furcâ tamen usque recurret."—Hor.

SECOND EDITION REVISED, WITH MATERIAL ADDITIONS.

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PREFACE.

"HUMANUM est errare."—The object of the present tract is to draw the attention of men of science, to what the author believes to be most pernicious errors in Chemistry, Electricity, and Magnetism; namely, that water is decomposable, that hydrogen is an elementary body, and that there are two kinds or states of Electricity and Magnetism; and to point out the true mode of action of these fluids.

If the errors in question should turn out to be mere fragile creations of the author's brain, no possible evil can result from this publication.

If, on the contrary, they should be found to have a real existence, of which the author has the fullest conviction, there can be no doubt that incalculable benefit must ensue by the removal of these mischievous intruders from the avenues of science, where they cannot but very seriously impede our approach to the sacred temple.

These considerations, the author trusts, will be received as an apology for his venturing to obtrude his ideas on the public attention.

Well House, Malvern Wells,
28th Sept. 1846.

PREFACE

TO THE SECOND EDITION.

The author has revised the former Edition, (which was written hastily) and introduced additional matter which he thinks will be found important.

He has also pointed out a further error which prevails in the doctrine of Combustion, and which he is of opinion may have contributed to the delusions which exist in modern Chemistry.

Brighton,
27th February, 1847.

MOST IMPORTANT ERRORS,

&c. &c.

NOTWITHSTANDING the eulogium passed upon Sir Humphry Davy by his admirable biographer, Dr. Paris, if we except the Safety Lamp,* we do not appear to be so much indebted to his labours as is generally imagined; nor has chemistry been left by him at all in a satisfactory state. He is said to have overthrown the theory of combustion of Lavoisier, and to have proved that oxygen is not the principle of acidity; but what has he substituted in their place? Instead of light and heat being properties of oxygen, which that substance parts with or developes at the time of its union with a combustible body, (according to the unfounded notions of the French chemist), Davy tells us that light and heat are the mere effects of motion, or, as he terms it,

* The efficacy of this lamp is due to the very unexpected discovery that flame will not pass through the interstices of a metallic wire gauze, (and which Dr. Paris allows a celebrated engineer claims to have found out before Sir Humphry), as the miners already knew that the "fire damp" would not explode *in the absence of flame*. It is then not to Sir Humphry Davy as a chemist, but as a mechanist, that we are indebted for this useful instrument.

of "intense chemical action,"* a doctrine certainly the least rational of the two; and with respect to the cause of acidity, he leaves it wholly unaccounted for. He is said, again, to have enriched science with some evanescent metals that are supposed to be the bases of alkaline earths; but which, for the reasons hereafter stated, will most probably turn out to be only compound bodies, as Mr. Curadon asserted, in his memoir read at the French Institute.

But what could the greatest genius effect with an imperfect knowledge of the materials upon which he operated, and of the tools which were to aid him? Now of what can these consist but of elementary substances,† and are we enabled to say that chemists are well acquainted with elementary bodies? If we are led from all experience to conclude that it is one of the great excellencies of Divine wisdom to arrive at the sublimest results by the simplest means, it would naturally follow, that to make any considerable progress in chemistry we should adopt a similar course. Instead of this, we have from fifty to sixty elementary bodies; so injudiciously have we, as it should seem, multiplied the four simple elements handed down to us from antiquity.

Many of these elements will certainly turn out to

* Whenever light and heat are elicited by any chemical change in bodies, it is exclusively due to the presence of the electric fluid. Vide *postea*.

† Chemistry is defined to be that science which investigates the "mutual agencies of the elementary principles of matter."

be an imaginary creation, resulting from the present imperfect state of chemistry; though the four in number, bequeathed to us by the enlightened Stageirite and *Princeps Philosophorum*, seem as restricted on the one hand as we have been profuse on the other.

The decomposition of compound inorganic bodies, by the aid of the galvanic current, has, however, shewn that the number of combining elements are very few.

It becomes, therefore, a consideration of the greatest moment that chemists should be perfectly assured that these few active matters are really the elements they are taken to be, and that no one of them is, on the contrary, itself a compound body; as it is self-evident that an error of this kind must not only overthrow the whole system of chemistry established since Cavendish's supposed discovery, but render all future experiments fallacious and delusive.

Now, I believe that I shall be able to shew that so far from there being any such assurance, a most fatal error exists with regard to hydrogen, and that it has arisen from a misconception of Cavendish's experiment, which instead of proving the decomposition of water, will be found only to establish the mode by which hydrogen is formed.

Another cause of this error may have been the little useful knowledge which has been acquired with respect to the electric fluid (elementary heat),

which, and not hydrogen, will be found to be an important element in all chemical combinations, and to exist in considerable quantities in metallic substances.

In the demonstration of these facts we shall arrive at the conviction of a phlogistic matter existing in all combustible bodies (the doctrine which Stahl maintained); and we shall find that the process of Combustion has not been understood, and that Lavoisier's theory, which attributes it to the combination of oxygen, as well as the subsequent theories, are altogether as erroneous as the supposed decomposition of water.

Whether the non-detection of these errors for so many years is to be attributed in a great degree to our public institutions, which (whatever may be their advantages) by engaging the energies of the professors in the search of novelty, in order to supply amusement as well as instruction for their auditories, necessarily withdraws them from that severe and contemplative consideration of experiments, and of their causes and effects, which the science of chemistry so imperiously demands, and without which the most mischievous consequences must ensue, I shall leave to others to decide. I cannot, however, dismiss from my mind the impression, that if the chemists, who succeeded Priestley and Cavendish, had been as meditative and profound as their predecessors, Boyle and Boerhave and Stahl, we should in all probability

have escaped those formidable fallacies with which chemistry seems to be at present surrounded.

It is true that our immortal countryman, Bacon, seeing the error of raising up systems without ascertaining the facts by which they were to be upheld, enforced the necessity of experiment for supplying these desiderata; and which, at the time he wrote, was admirable advice. But we now fall into the other extreme, and are daily multiplying experiments and producing results without having any clear or definite object—thus making, as it were, "confusion worse confounded."

In fact, modern chemistry looks very much like a scramble for popularity. Instead of pondering over and closely scrutinizing some few of the million of experiments already made, which call for investigation, it seems to be the only question, who can run the fastest on the road to novelty; and he who contrives to pick up some wild apple in his route, which has a little more colour in its cheeks than ordinary, is forthwith crowned with public applause, to the great envy and disappointment of his breathless competitors.

These remarks apply equally to electricity: for with the present most romantic and visionary notions of the nature and properties of this matter, and which might entitle them to a place in the "Arabian Nights' Entertainments," how otherwise can we regard the so-called beautiful, and other similar experiments for which gold medals are claimed (if

we are to use the stern language of truth), than as so many amusing results of persevering ingenuity?

Preliminary to our examining the experiments relative to the "decomposition of water," it may be curious to consider what grounds there are, independently of chemistry, for holding this fluid to be an elementary body.

We find, then, in the first chapter of Genesis, that "God created the heaven (the air) and the earth," and afterwards "that the spirit of God moved upon the face of the waters." Again, that "God said, Let the waters under the heaven be gathered together unto one place, and let dry land appear. And God called the dry land earth."

From these passages it would seem (and the contrary is not stated in the rest of Genesis) that the water must have existed at the time the heaven and the earth are spoken of, and was not a contemporary creation, as no mention is made of the creation of water.

The word creation, applied to the air and earth, does not, *ex necessitate*, imply that these were not compound matters. It is quite consistent with the sense that they should be each a combination of different elements, as they are found to be. But the superiority of the water over these two bodies is to be concluded from the emphatic passage quoted above, which says that "the spirit of God moved upon the face of the waters." Now, this superiority could only consist, apparently, in its being the sole

uncompounded matter of the three mentioned constituents of our Globe---viz. air, earth, and water.

There is one other passage, which though not bearing immediately upon the point under consideration, yet I will beg leave to quote, as it is not a little remarkable, and has hitherto, I believe, escaped notice—viz., "And God said, Let there be a *Firmament* in the *midst* of the waters, and let *it* divide the waters from the waters." Now, it is a curious circumstance that the Hebrew word which is translated into "Firmament," signifies *Expansion*,* so that the true reading would be, "And God said, Let there be *expansion* in the *midst* of the waters, and let it (the expansion) divide the waters from the waters;" from which it is to be inferred that the earth was a *nucleus* enclosing an elastic fluid† (most probably elementary heat or electricity), surrounded by the waters, and from which it partly disengaged itself by *expansion*, and thus caused dry land to appear.

If then we may be allowed to regard the six days as typical only of the periods of the formation of our planet and of the changes it has undergone by expansion, preparatory to the existence of the human species, we shall have the satisfaction of placing the Holy Scripture and the Geologists in perfect accord.

* See note on the word "Firmament," 1st chap. Genesis, v. 6. Oxford edit. 1831.

† ἐλαστικος, *from* ἐλάω, *ago, agito, impello*.

Now, then, let us consider this question upon the ground of probability. I have already observed that the highest conception we can have of Divine power, is that of producing the most splendid results by the simplest means; and whenever we are able to comprehend any of the processes of nature, we find this invariably to be the case.

Is it probable, then, that the water of our globe, three times in extent to that of the earth, and which, at the time of the creation, must have been still more capacious, as much of it must have been volatilized to form an atmosphere;* is it probable, I ask, that so important a body to the existence and comfort of the inhabitants of the earth, and on that account so bountifully supplied, should be a compound of gaseous substances, the quantities whereof to form such a matter far exceed the stretch of human imagination, the mean depth of the ocean, according to Laplace, being not less than from four to five miles?

We may, perhaps, be able to form some very faint idea of the innumerable oceans of these gaseous matters which would be necessary to compose these waters, when we learn that Cavendish by combining 500,000 grain measures of hydrogen, and about 1,500,000 like measures of atmospheric air, produced *one hundred and thirty-five* grains of water.†

* Davy says, "The atmosphere always contains water in the elastic and invisible form, varying in quantity with the temperature."

† Phil. Trans. Abr. vol. 15, p. 437.

We cannot, therefore, suppose such a combination of gaseous matters, without adopting the unphilosophical notion of an overwhelming necessity.

I am aware, it may be said, that the Deity could have effected the combination of these gaseous matters by the mere expression of his will. To this, it may be replied, that the same will might have made the water an element, which I believe to be the fact. But we also know that the Creator, in his infinite wisdom, has thought proper to limit his power over matter, by assigning to it certain properties, called laws, and that it is by the invariable observance of these laws that the universe is governed.

We know this from the accumulated observation of ages; and it is to this important knowledge, that we are able to banish from our minds the long prevailing belief in a frequent supernatural agency, with its hosts of miracles; a doctrine which has been found to be as pernicious to religion, as it is offensive to philosophy.

We will now proceed to an examination of the experiments by which the decomposition of water is said to be established.

It will be but just to premise, that the investigation of an important process, and the repetition of such process, by an *advocate* of the doctrine, is a very different affair from the scrutiny and repetition of the same experiment by an opponent, and this, notwithstanding there should exist not the remotest doubt as to the *bona fides* of the operator.

I cannot enforce this remark better than by quoting the sentiments of Sir Humphry Davy upon this point in the fifth dialogue of his "Last Days of a Philosopher." "By *often* repeating," he says, "a process or an observation, the errors connected with hasty operations or imperfect views are annihilated; and, *provided the assistant has no preconceived notions* of his own, and is *ignorant of the object of his employer* in making the experiment, his simple and bare detail of facts will often be the best foundation for an opinion."

Now, we collect from this, the extreme danger of being deceived by an experiment, (though the greatest apparent care may have been used) wherever the operator's mind has been imbued with the idea of what ought to be the result of such experiment; and infinitely more so must it be where the good wishes of the operator have accompanied the expectation of such a result.

I feel these remarks necessary, as the observations of Davy strongly apply to himself, as we shall presently see, as well as to the processes connected with the decomposition of water, and which can by no means, therefore, be implicitly relied upon; and the less so, as there exists an extreme difficulty in satisfactorily conducting the experiments, as will presently appear.

The experiment consists in putting *pure water* into a glass vessel hermetically closed, and by the introduction of the electric or galvanic fluid decomposing the water in question, so that upon the

termination of the process, no water is found in the vessel, but in lieu thereof, hydrogen and oxygen gases, in the proportion of two volumes of the former to one of the latter, according to Davy ; but, according to Cavendish, only one of hydrogen and five or six of oxygen.*

* M. Dumas, the celebrated French chemist, says, that one part of hydrogen and *eight* parts of oxygen form one atom of water. See *Annales de Chimie, June*, 1842.

In the first edition of this pamphlet I stated, that a discrepancy existed in the writers on the decomposition of water, with respect to the quantities of the gases to be united.

Again, in a subsequent page, (it having occurred to me that it might be asked why I produced no experiments of my own,) I *admitted* that I was not a practical chemist, and had *never* made any.

A Professor of Chemistry, as I have reason to believe, in one of our public institutions, who, from injudicious applause improvidently repeated, seems to imagine himself a Phœnix of scientific knowledge, thought proper to publish in a popular weekly publication, the Athenæum, a violent invective on both these statements.

With regard to the first, he says, that some of the writers speak of the quantities by measure, and others by weight. Now, admitting this to be true, (and which, from its causing confusion, had better have been avoided in so important an experiment,) still it does not remove the discrepance between Cavendish and M. Dumas. It is, however, unnecessary to discuss the point farther, as the decomposition of water does not turn upon the quantities of the gases, but mainly upon the simple fact, whether hydrogen is an elementary or a compound body.

With regard to my second statement, the learned professor chose to make it a subject of personal attack, and to charge me

This is said to have been effected, first by Cavendish, and subsequently by Lavoisier, Wollaston, Davy, &c., all of whom, be it observed, gloried in the discovery; and, indeed, the two last, in common with many other celebrated characters, considered Cavendish as having by this process contributed additional rays to the splendour which Newton had already shed upon this our highly-favoured country.

To render the process in question decisive, two things are indispensable, viz. that at the commencement of it, the water should be wholly free from oxygen; and that, during the operation, no oxygen should find its way into the vessel.

Now, I think it will clearly appear, that none of the experiments made upon water have been free from these objections, in which case I submit they are valueless as evidences of any decomposition having been effected.

To shew the extreme difficulty, nay, the almost

with having admitted an extent of ignorance so *crass*, as to disqualify me altogether from belonging to the Society of which I have the honour to be a member.

But, unusually coarse and ungentlemanly as this attack must be admitted, I shall withhold my reply until this wise professor shall first establish, that no one can judge of a painting but an artist; or of a literary work but an author.

In the meantime I will just whisper in the ear of this Professor of Chemistry, that it is not experiments of which we stand in need, *but of men capable of comprehending them when they are made.*

impossibility, of obtaining pure water, and when supposed to be so obtained, of preventing the ingress of oxygen with the galvanic fluid, I will quote the following experiment, taken from Davy's Bakerian Lecture, delivered on the 26th November, 1806.

Mr. Sylvester had asserted, a very important circumstance, viz.: that if two separate portions of water were electrised, out of the contact of substances containing alkaline and acid matter, acid and alkali would nevertheless be produced.*

As Mr. Sylvester's experiments were supposed to establish these facts, "some persons," says Dr. Paris, "thought that *the salts contained in the fluids of the troughs of the voltaic pile might, by some unexpected channel, find their way into the water under examination.* Others, *that they were gene ated by the union of the electric fluid with the water*, or with one or both of its elements."

Davy's anxious desire was, however, to test, and if possible, overthrow Sylvester's experiments, it being evident that if Sylvester was right, great

* M. de la Rive, the ingenious Professor of Chemistry at Geneva, says, that in all the experiments on water by the voltaic current, *oxygen* is disengaged at the surface of the metallic poles, arising from minute particles of metal oxydated, and which are suspended in the current, and are thus conveyed into the vessel containing the water; and M. de la Rive, be it observed, is a believer in the decomposition of water.—See *Archives de l'Electricité*, by M. de la Rive, a work which contains all the experiments upon that subject.

doubt would arise with respect to the accuracy of many chemical experiments, including particularly that of reducing water into its supposed elements.

For this purpose, and to avoid all possible impurity in the water, Davy used two small cups of agate, which were boiled for several hours in distilled water, and a piece of very white and transparent amianthus (a substance first proposed by Dr. Wollaston) similarly purified, was made to *connect* the vessels together.

"Thus we see," says Davy, "that every apparent source of fallacy was removed; but, nevertheless, after the *purest distilled water* had been exposed in the agate cups to the voltaic current for forty-eight hours, the water in the positive cup gave indications of muriatic acid, and that in the negative cup of soda."

Thus, then, had oxygen either existed in the water, and formed the acid; or it must have resulted from the acid mixture in the trough, or from oxydated matter transmitted by the poles of the voltaic battery.

The experiment was therefore repeated by Davy a second, third, and fourth time; "the agate cups having been carefully placed in glass vessels, out of the reach," says Davy, "of any circulating air; and all the materials having been repeatedly washed with distilled water, and no part of them in contact with the fluid having ever touched the fingers—*but still the same result.*"

"The experiments were, in consequence," says Davy, "again repeated, and instead of agate cups, small cones of the *purest gold* were used, and the water contained in them submitted to voltaic action for 14 hours; the result was, the water in the positive cup *became acid, which increased in quantity as the experiment proceeded; and at length became sour to the taste.* On the contrary, the alkaline property of the fluid in the opposite cone shortly obtained a certain intensity, and became stationary. The acid, as far as its properties could be examined, agreed, says Davy, with those of pure nitrous acid, *having an excess of nitrous gas.*"

With these results Davy was again dissatisfied. Now let it be particularly remarked, that you have here a chemist exerting all his energies to overthrow a theory which he thinks proper to oppose; whereas, in the experiments on the decomposition of water, you have all the chemists exerting themselves to confirm a theory by which they suppose they are to acquire an additional element to enrich the stores of chemical science.*

This forcibly illustrates the pungency of Davy's remarks, of the necessity of the operator having no preconceived notions about the result, and, at all events, that he should be exempt from any prejudice or wish upon the subject.

Davy now submitted the *water* to a still more

* Hydrogen;—Dr. Priestley had previously discovered oxygen.

rigorous examination, which he did by *evaporating* it in a silver vessel, when he discovered 1-70th of *a grain* of saline matter.

The water thus purified was again subjected to a voltaic current in the cones of gold.

"In *every one of these experiments*," says Davy, "*acid matter was produced* in the positive cup, and always with the character of *nitrous acid*. How this acid could arise," he says, (determined, as we see he was, that Mr. Sylvester should not be right) "he could not imagine. It occurred to him at last that the *nascent oxygen and hydrogen* of the water might combine with the *common air which is constantly dissolved in that fluid*;" (and this, observe, he supposes possible, notwithstanding the water had been so carefully purified)—but how did it happen, he adds, that the production of *nitrous acid was progressive?* Davy then remarks, that he recollected some experiments of Priestley on the absorption of gases by water,* and of the difficulty of their exclusion, and he therefore introduced the two golden cones containing the purified water under the receiver of an air pump, when the exhaustion was effected, and the voltaic pile brought to act upon the water thus further purified. After 18 hours, the result was examined, when the water in the negative cone produced no effect upon prepared litmus, but that in the positive cone *did give a tinge of acid*, barely perceptible.

* The solvent and absorbent powers of water are now perfectly well known.

Thus ended the process. But there is every reason to suppose, I submit, from his previous experiments, that had he *continued* the voltaic current for 48 hours instead of 18, the water, notwithstanding the action of the air pump, would have gone on *progressively increasing in acidity*. Indeed, it must be admitted, that in candour he ought to have continued the last process as long as he had done the first, when he used the agate cups.

We see, then, from these experiments, that the ability of the most able artist in chemistry is incapable of excluding oxygen from entering into the vessels, and combining with the water.*

There can be no doubt, indeed it is admitted, that water in its pure state is neutral; that it contains neither acid nor alkali: and it seems equally indisputable that these matters are conveyed to the water by the voltaic current, and that the proportion of alkali is very trifling compared with that of the acid.

It is equally plain, that if an acid is communicated by the voltaic fluid, the longer the operation continues, the more the acid will impregnate the water; and this appears to be established by the experiments we have just noticed.

I now confidently appeal to every impartial che-

* If the water in Davy's first six experiments was free from oxygen, it is evident that Sylvester is right, and that oxygen is (as M. de la Rive also insists) carried with the electric current, and thus enters the vessel containing the water. If, on the other hand, the water always contained oxygen, then no satisfactory conclusion can be drawn from the experiments on water.

mist, and ask, whether in any of the processes recorded, either of Cavendish, or any other of the chemists who have repeated his experiments, the water acted upon by the voltaic current was in anything like the state of purity; or whether anything like the same means were taken to produce such purity, as we find to have been adopted in the processes I have just mentioned? and if such has not been the case, I ask what satisfactory evidence exists of water being decomposed? But supposing no objection to be taken to the purity of the water, is there any doubt that it becomes charged with oxygen through the operation of the galvanic current, and acquires an acid taste? Who can say, then, that he is now perfectly satisfied that water has been decomposed?

I will now produce a strong and direct argument from Davy himself against any such decomposition.

In considering the action of the voltaic pile, Davy says—"He thought that *if the fluid medium of the pile, could be a substance incapable of decomposition,* there is every reason to believe the equilibrium of the two opposite metals would be restored, *and the motion of electricity cease.*"*

Now, at a subsequent period we find him stating —"that *pure water* does not act upon the voltaic pile;" and he makes this observation, "the galvanic pile only acts as long as the water between the plates *holds some oxygen in solution.*" *Has he not*

* Dr. Paris's Life of Davy, vol. i. 246.

*found then in water the indecomposable "fluid medium,"** the effect of which, he says, will be to suspend the motion of electricity?

According to Davy, if water were decomposable, and particularly if oxygen were one of its constituents, it would be decomposed by the action of the metallic plates—as he supposes the operation of the voltaic pile to depend upon the fluid's being a compound containing oxygen; instead of which the plates cannot eliminate any oxygen from it.†

But a still stronger, and as it appears to me, unanswerable argument against any such decomposition is this. In the process just mentioned we find that Davy connected the agate cups and golden cones together with amianthus, and placed them in glass vessels hermetically closed; and that the small quantities of water contained in these agate cups were submitted to the action of the powerful voltaic pile of the Royal Institution (then consisting, I believe, of two thousand double plates) for forty-eight hours, and which experiment was repeated a second, third, and fourth time; and that the water

* Ib. vol. ii. 210.

† This circumstance alone raises the strongest possible presumption against oxygen being one of the elements of water.

Davy was right in saying that water would not act on the plates of the voltaic battery, unless it contained oxygen; but he was wrong in imagining that the cause of the inaction of pure water is, that it restores the equilibrium of the two opposite metals; the fact being, that electricity is evolved by the decomposition of metals, and that to effect such decomposition an oxigenated acid mixture is necessary.—Vide *postea*.

in the golden cones was so acted upon for eighteen hours, and yet that these insignificant portions of water stood this intense voltaic action *without being decomposed!*

It is evident that Davy, in the ardour of his pursuit, and the exultation of a purely imaginary victory over Mr. Sylvester, (for no impartial man can believe it to have been really gained,) never once reflected that he was giving conclusive testimony to the fallacy of the experiments in which water had been thought to be decomposed.

He speaks, indeed, as we have seen, about a *conjecture*, that there *might be* some *nascent hydrogen and oxygen*, wholly forgetting that according to Cavendish, Lavoisier, Wollaston, and himself, the whole of the water in each of these operations ought to have been resolved into "thin air."

Now these are objections which present themselves to the experiments on the decomposition of water, *upon the supposition that hydrogen is an element;* but we shall find that hydrogen is not an element, but a combination of phlogiston (electricity) and water. What, then, becomes of Cavendish's discovery, the belief in which has contaminated the whole system of chemistry, and rendered the experiments* of the last 60 years one heap of ruins, "in dire confusion piled?"

It has been attempted to anticipate my objection that hydrogen is a compound of electricity and water, and that this combination takes place when

* Chemical Analyses.

the water is supposed to undergo decomposition, by a learned Professor of Chemistry having insisted *that water can be decomposed by heat alone*, and without the presence of electricity,— viz. *by passing the water through a red hot iron tube.*

This is, however, a total misconception; for I shall satisfactorily shew that electricity is an element of all metals, and that it is liberated therefrom whenever oxidation takes place either by heat or through the agency of an acid, or any other such matter.

Mr. Grove not being aware in what way the electricity of the voltaic pile is generated, or rather the source from which it is derived, also imagined that he had effected the decomposition of water by heat exclusively, when instead of connecting the platinum wire (communicating with the water acted upon) with the voltaic battery, he brought it into a white heat by means of a spirit, or oxyhydrogen flame, and a blowpipe. The vessel, too, containing the water was, it is to be remarked, of metal, and not glass.*

That the electric fluid is an element of metals, is evident from the mode in which it is generated in the voltaic battery. Metals are there used (of opposite conducting powers), which are the *most easy of oxidation;* and to produce this oxidation an oxigenated water, or acid mixture, is employed, the

* See the Report of the Meeting of the British Association at Southampton, in October, 1846.

result of which is that electricity is evolved in large quantities. Now, whence can this electricity be derived but from the metals? But if this were doubtful, the doubt is at once removed when we find, (which is matter of notoriety) that metallic calces are brought back to the metallic state upon having an electric current passed through them. Can any proof be more clear or satisfactory?

It is also matter of notoriety that electricity is found in considerable quantities in mines, and in those parts of the earth where metals exist. There are also other indisputable proofs of the same thing, which we shall learn as we proceed.

That the electric fluid will combine with a mineral substance as well as with metals, is well established by what occurred at Chambery, as will be seen from the following Report:—"On the 14th June, 1846, a church at Chambery was struck by lightning. The interior was filled with a black smoke, and the smell was compared to that of gunpowder. Mr. Beaujean being informed that certain gilt articles had been blackened by the electric fluid, examined the place the following day. He found the transverse portions of the gold frame of a large picture almost entirely blackened, as well as six gilt chandeliers, just as copper would be when immersed in sulphuretted hydrogen gas. A portion of the dark film was scraped off one of the chandeliers, and digested in pure nitro-muriatic acid. The addition of a salt of barytes to the diluted solution

indicated clearly the presence of sulphuric acid. It would thus appear (says the reporter) that the electric fluid was accompanied with sulphur in some state of combination, probably as sulphuretted hydrogen. The odour, therefore, so frequently observed, is probably due to the presence of the above-mentioned gas."*

When we find electricity to be an element of metals, and to combine with mineral substances, there seems no reason why it should not combine with the atoms of a fluid body.

Dr. Priestley, in fact, proves that electricity will combine with fluids, as he found that an *inflammable gas* was evolved by passing a current of electric sparks through olive, or any other kind of oil.

Again, Macquer, in 1776, found that by holding a china saucer over the flame produced by the *combustion of hydrogen gas*, drops of water were produced.

Is not this last experiment decisive of the fact that hydrogen gas is composed of water and a phlogistic matter?

If it be said that the oxygen of the atmosphere united with the hydrogen and formed the water, my reply is, that this is wholly a misconception of the experiment, which is, as we shall see presently, one of *pure combustion*. The hydrogen in this case was manifestly burnt, by being ignited in the presence of atmospheric air, when, as invariably fol-

* See Comptes rendus of July, 1846.

lows, the combustible body was destroyed (viz., wholly decomposed), and the water, one of its elements, was consequently restored.

In what way, let me ask, is hydrogen, when wanted in considerable quantities, ordinarily obtained? By dissolving zinc or iron filings in *dilute* sulphuric acid, when an effervescence takes place, and hydrogen is evolved.* Now, what is the *rationale* of this process, but that a decomposition of the metal takes place (as in the voltaic battery) followed by the liberation of electricity, and which combining with some of the water of the diluted acid, forms the hydrogen.

Is there any other mode of accounting for the hydrogen in this process? Iron and zinc are not composed of hydrogen,† nor does it exist in dilute sulphuric acid. But from the way in which it is produced, it must, *ex necessitate*, be a compound body; but of what compounded, if not of the matters I have stated?

Dr. Priestley says he procured hydrogen by distilling well burnt charcoal, when he obtained hydrogen and carbonic acid in nearly equal quantities. Now, charcoal is a powerful conductor of electricity, and largely charged with that fluid,‡ so

* Cavendish says he obtained all his hydrogen from zinc.

† Dalton, in his Atomic Theory, says he adopted hydrogen as unity, from its uniting in the *smallest* proportions with other bodies. Vide *postea*.

‡ Lavoisier and Laplace, assisted by Volta, found that *strong electricity was generated by burnt charcoal*, and that by increas-

that the *rationale* of this experiment will be the same as the last. In what way can it otherwise be surmised that distilled charcoal can produce hydrogen?

But Mr. Pouillet, of the French Institute, found that not only the flame of charcoal yielded electricity, *but that the flame of hydrogen did the same.*

That the flame of hydrogen gives out electricity, does not rest alone upon Mr. Pouillet's authority; Priestley had found long before that hydrogen gas gave out electricity without flame, as he says he reduced (i. e., brought back or revived) the calces of metals by *heating them in hydrogen gas.**

It is true that Priestley was not aware that the revival of the metallic calx was due to electricity; but since Beccaria's experiment the fact is known to be so, and Beccaria's experiment has been often repeated.†

So clear was Priestley that hydrogen was derived from metals, that he calls that gas "*the inflammable air from metals;*" and so does Watt. ‡

If then hydrogen is a body compounded of electricity and water, and which, as far as any conclusion can be drawn from the mode of procuring it,

ing the quantity a spark could easily have been obtained.—See Ency. Britan. 8 vol. pp. 599 and 606, for this and Pouillet's experiment.

* Calces of metals are also revived by placing them in hydrogen gas, exposed to the rays of the sun.

† See Note, p. 32.

‡ See Watt's paper, Phil. Trans. Abr. 15 vol. 566. The French chymists had observed that hydrogen is found in metallic mines. See Memoires Paris Acad. for 1746, p. 286.

with the circumstance of its flame giving out electricity, leaves that fact incontrovertible, we are evidently driven to adopt the wrongfully, and unfortunately exploded doctrine of Stahl, who maintained that the combustibility of bodies was owing to their combination with a principle of elementary heat, which he called Phlogiston. He also insisted that combustion took place by the *escape* of that subtile element. In this latter assertion, however, he was mistaken, as we shall presently perceive.

My theory, however, in bringing back the principle of the Stahlian doctrine, (viz. elementary heat), presents this additional and important fact, that this phlogistic matter can be no other than the electric fluid.

Now let us consider for a few moments upon what ground Stahl's theory was supposed to be overthrown.

In 1774, Priestley discovered oxygen; and it was found that when a metal was calcined (oxidated) it lost its metallic lustre, but increased in weight.

It was thereupon said by Lavoisier that the theory of Stahl must be wrong, as if combustion (the oxidation of metals was, as we shall see, erroneously so called), were caused by the escape of the phlogiston, the *residue* of the metal would be necessarily lighter, and not heavier; and as it was ascertained that in the process of combustion (oxidation) the metallic calx had combined with the oxygen, the latter was concluded to have caused *both* the increase of weight and *the combustion.*

Now this reasoning, which imposed by its speciousness, is nevertheless fallacious.

For, in the first place, what is meant by the combustion of a metal by oxidation?

The metal having under certain circumstances a greater affinity for oxygen than for phlogiston (electricity, and one of its elements, as we have seen), parts with the latter principle to combine with the oxygen.

By this change of the elements, the matter has ceased to be a metal. Its lustre is gone, and the matter left is heavier than in its original state. The cause of the additional weight is perfectly clear, inasmuch as the phlogiston which has quitted the metal is an imponderable body, while the oxygen which has taken its place is a ponderable substance.

But how does this, in the remotest degree, disprove the existence of a phlogistic matter in the metal before it became oxidated?

On what just ground then has the Stahlian doctrine of a phlogistic principle been abandoned?

That a phlogistic matter is in the metal, and that this matter is electricity is evident; for by the loss of this principle that which was a metal ceases to be so.

But it will be asked, how do you prove that this change is due to the loss of the electricity as well as to the combination of the oxygen? I reply that this is unanswerably established by the fact that upon re-

ceiving the electric current, a metallic calx becomes again a metal.*

The simple fact then is, that by the oxidation, the metal has merely exchanged one element for another, (the oxygen for the electricity), and like other compounds under similar circumstances, it has by such exchange become another substance, called a metallic calx or oxide.

But this is by no means combustion in the ordinary and proper sense of the word, any more than water may be said to be burnt when it becomes acid by its union with oxygen.

Lavoisier has clearly confounded two distinct operations. Having found that oxygen is always present in what I shall call, the violent or ordinary decomposition of a body by means of extreme heat, he erroneously concluded that combustion takes place *whenever* matter combines with oxygen.

The two processes, however, are wholly and substantially different. In combustion, truly so called, the matter acted upon is (in popular language) destroyed, or *entirely* decomposed, by the act of *superadding* a phlogistic matter to that *already existing* in the combustible substance; but in the oxidation of a metal *no additional phlogiston or heat is supplied;* on the contrary, the phlogistic principle which the metal contained has quitted it to give

* Vide Beccaria's Artif. Electr. also. Ency. Metrop. "Mixed Sciences," vol. 2. p. 112. When a strong electric current is passed through the calx, the oxygen is driven off, and the electricity takes its place. This is evident from the calx recovering its lustre, and becoming specifically lighter.

place to another element not combustible (oxygen); and this is all the decomposition the metal has undergone.

So far therefore from combustion having taken place by oxidation, the metal by the exchange of its phlogistic principle for oxygen, a non-combustible, has necessarily become *incombustible;* and it cannot become again combustible without the restoration of the elementary heat. If, however, the phlogiston be restored to the calx or oxide;* or if a metal in which oxidation has not taken place, (which is the same thing) be placed in a vessel containing oxygen gas, it may be burnt (entirely decomposed) either by subjecting it to a galvanic current, or electric sparks, or by *previously igniting the metal;* for no combustion or complete decomposition of a combustible body can, I repeat, take place without the application or aid of *additional phlogiston* to that already existing in the combustible body.

Three things are essential to effect combustion.

1st. A combustible body, i. e. matter united with phlogiston, (electricity.)

2ndly. The presence of the pabulum of fire, oxygen.

3rdly. Extraneous or additional phlogiston.

It is the *superabundance or excess of phlogistic matter,* with the aid of oxygen, which causes the combustion of a metal or other matter, and not the simple abstraction or escape of that principle, as in the process of oxidation.

* By submitting it to an electric current.

It is to this oversight of Lavoisier's that we may probably owe the delusion of the composition of water, as in the grand experiment of Cavendish, by which water is supposed to be the result of the union of the two gases, it is the Theory of Combustion which is exemplified, and not the combination of hydrogen and oxygen as he imagined.

For instance, hydrogen (inflammable air) Cavendish believed to be a phlogistic matter and water, and from which opinion *he never deviated.** Now this phlogistic matter being combined with as much water as it will unite with to form the so-called phlogisticated air,† it will necessarily not be affected or altered by any additional Electricity; and which Priestley found to be the case. But if oxygen, or

*It having been asserted that *inflammable air was pure phlogiston*, Cavendish says, "I know of no experiment which shews inflammable air to be pure phlogiston, *rather than an union of it with water*, unless it be Doctor Priestley's experiment of *expelling inflammable air from iron by heat alone*. I am not sufficiently acquainted with the circumstances of that experiment to argue with certainty about it; but I think it *most likely* that the inflammable air was formed *by the union of the phlogiston of the iron filings with the water dispersed among them, or contained in the vessel in which it was heated*, as iron seems not to be disposed *to part with its phlogiston by heat alone without being assisted by the air* (oxygen) or some other substance."—Phil. Trans. Abr. vol. 15, p. 492. It is clear that Cavendish considered the inflammable air to be a compound, *and that the phlogiston was derived from the metal.*

James Watt says, "It is my opinion inflammable air contains *a small quantity of water and much elementary heat.*"—Phil. Trans. Abr. vol. 15, p. 555.

Priestley admitted, in his subsequent writings, that Cavendish had satisfied him that his notion of having produced hydrogen from metals by *heat alone and without water* was erroneous.

† Hydrogen was called indifferently, inflammable or phlogisticated air.

atmospheric air sufficient to furnish the amount of oxygen required, be added to it, it is then in a state to undergo the process of combustion by the introduction of *further phlogiston*, such as an ignited wire, a flame, or electric matter, &c.

Now this is precisely the case of the gas which we meet with in the mines. Instead then of the *explosion* which takes place being, as has been supposed, the result of a *combination* of the two gases, it is simply the combustion or decomposition of the hydrogen, which arises from the addition of fresh phlogistic matter applied to the hydrogen in the presence of oxygen: a process perfectly identical with that of all the processes of combustion properly so called.

The consequence of this decomposition is the escape of the electricity, and the restoration of the water.

That it is the combustion of the hydrogen which takes place, is confirmed by the use made of these gases in decomposing bodies by the aid of the blow-pipe: as Dr. Clarke says, that he found the *greatest heat* was produced by the deflagration of two portions of hydrogen with one of oxygen; (the exact proportions said to form water) a heat, says the Doctor, *greater than that produced by the largest galvanic battery.**

* See Professor Clarke's Treat. on the Blow-pipe. Also Journal of Science of the Roy. Inst., October 1816. It is found that when these gases are projected through very small apertures against a flame, the decomposition of the hydrogen is not accompanied by any apparent explosion; and the heat is too intense to allow of any trace of humidity.

Let us now take into consideration the gas of the coal-mines, called "fire damp." This matter, according to Davy's analysis of it, is "carburetted hydrogen,"* which, he says, by the admixture of *not less* than six times its volume of atmospheric air -- explodes; and the result of which explosion is, as we know, that the vaults or passages of the mine *run down with water.*

It will be important then to look at the quantity of this carburetted hydrogen found in the mines. In one colliery alone, belonging to the Lowther family, the miners had picked a hole, from which *a uniform current* of the gas continued to issue for the space of two years and nine months;† and out of the fissures which the men had cut, there had issued *seven hundred hogsheads of carburetted hydrogen in a minute; and this quantity of gas continued to be emitted for years.*

Whence comes this most extraordinary quantity of hydrogen? When we find that the separation of the elements of water is attended with the difficulty we have recently seen,‡ can any man believe that the hydrogen gas in question is the result of any such decomposition?

But a still more embarrassing question is this. If hydrogen gas be one of the elements of water, *what has become of the other?* For if 700 hogsheads

* Davy found four of hydrogen, and 11½ of charcoal formed the carburetted hydrogen; the hydrogen being somewhat more than ⅓ of the composition.

† Philos. Trans. vol. 38. p. 113.

‡ See page 12.

of hydrogen gas issue in a minute, about 250 hogsheads of oxygen ought to accompany it, according to Davy; while not one hogshead, I believe, is to be found in the whole mine.

Can any rational man require the subject to be carried further in order to prove that the hydrogen which composes the fire-damp, has never been generated by the decomposition of water?*

We will now recall to our recollection, what the ablest of our scientific chemists have themselves thought upon the subject of hydrogen. It is ascertained that Watt, to the latest moment, agreed with Stahl, and "*regarded heat* as *material*, and to have the capacity of *combining with substances like other material elements.*" He even went farther, and gave it as his opinion that "inflammable air" (hydrogen) *contains a small quantity of water and much elementary heat.*"† Priestley says, "that the water resulting from the explosion of hydrogen and common air, *he thought to be water held in mechanical solution.*" Priestley adds this important remark—viz., that when Cavendish's paper on the formation

* If it be said that this hydrogen is independent of any decomposition of water, I should like to know how the chemists account for the presence of this gas in quantities so largely disproportionate to their other active elements, when it is known to unite in the *smallest* quantities with other bodies? See note, p. 44.

† Watt says, that he "concluded from his experiments that *heat was a combining substance*, not merely modifying the form and condition of *elements*, but determining likewise *their permanent specific heat.*" See Watt's paper already referred to.

of water was read to the Royal Society, Cavendish told him "he was persuaded *that water was essential to the production of inflammable air.*"*

It appears, as we have seen from other papers of Priestley, that he had at first concluded from several of his experiments, *that inflammable air* (*hydrogen*) *was separated* from metals, such as zinc, tin, or iron *by heat alone ;* but he adds, that in all such experiments, *Cavendish* showed him *that there was water present* in some form decomposed, and told him that water was essential to the production of inflammable air.†

If the experiment of Cavendish in the supposed combination of the two gases is simply that of the combustion of hydrogen, and which cannot now, I should think, be a matter of doubt, we know what must be the result—viz., that the electricity escapes, and that the oxygen and water are left behind, when the latter will absorb the oxygen and consequently become acid.

Now this is precisely what Priestley states to be the consequence of the (supposed) combination of the two gases in question. Priestley says he often repeated Cavendish's experiment, and that he found invariably an *acid mixture* to be the result, and not

* See Priestley's paper in Phil. Trans. *postea.*

† See Priestley's Works, vol vi. p. 87. In a very able article in the Quarterly Review for Dec. 1845, written to shew that Cavendish was the first discoverer of the decomposition of water, references will be found to the above quotations if they should be required. See the Quarterly Review for December, 1845, 105.

pure water; and *he denies that pure water can be obtained by such combination.**

Indeed, in the papers of Cavendish, which will be hereafter cited, he himself admits that the acidity of the water depends upon the quantity of oxygen employed in the experiment.

But Priestley will be found to go still further; for he states that on looking carefully at the vessel when the combustion takes place, it is visible to the naked eye *that another matter* in the form of a vapour exists *independently of the water.*†

That since Priestley's time this momentous subject of the decomposition of water has not received that close investigation which so weighty a question demands, but has always been treated as *un fait accompli*, will be seen from the anecdote related by Mr. Babbage in his "Reflections on the Decline of Science in England," and quoted by Dr. Paris. "All gases," says Mr. Babbage, "being reducible to a liquid state by compression, I proposed to Sir

* See Dr. Priestley's papers, vol. 16, Phil. Trans. Abr. p. 318, and vol. 17, p. 55. Also see Priestley's works, vol. 6, p. 87. Dr. Priestley always denied that *pure water was ever produced* by the combination of hydrogen and oxygen. He observes that in *all* his experiments an *acid liquor* was the consequence. In these experiments of Dr. Priestley there can be little doubt that the acidity was occasioned by the oxygen uniting with the water which had resulted from the deflagration of the hydrogen, as the action of the voltaic current would not have been long enough to supply any oxygenated matter aliundè.

† The oxygen before its uniting with the water. See vol. 16, Phil. Trans. Abr. p. 518.

Humphry Davy the question, whether, if two volumes of hydrogen and one of oxygen are mixed together in a vessel, and if by *mechanical pressure* they can be so condensed as to become of the same specific gravity of water, the gases will unite and form water. Davy at once said, 'They will become water of course;' and on my inquiry whether the experiment would not be worth trying, he replied, it was hardly necessary to make it, *as it must succeed.*" On the same question being put by Mr. Babbage to Dr. Wollaston, he was of a contrary opinion, assigning as the cause the *nature of the electrical relations of the two gases remaining unchanged.*

We cannot, however, believe that the inquiring mind of Dr. Wollaston would pass over an experiment of this kind, and we may, therefore, conclude that it was subsequently tried by him and did not succeed. But the reason assigned by him for its failure was at variance with the existing notions of electricity—viz., that oxygen is charged positively and hydrogen negatively, and which are the electric states the most favourable for combination: in fact the only states that are thought to do so.

But assuming that they were not so, what ground had Wollaston for supposing that when the two gases were enclosed in a vessel any additional electricity would disturb the *nature of their electrical relations?* —and if not, why does mechanical pressure fail?

The late Mr. Biot, of the French Institute, however, resolved this question, and found that by consider-

able compression he could combine the hydrogen and oxygen, when they gave out light and heat, but were not converted into a liquid. He adds, that he first tried a pressure of 30 atmospheres by sinking a vessel of the mixed gases in the sea to a depth of 150 fathoms, which proved insufficient.*

But there yet remain other objections to Cavendish's theory.

Assuming for the moment that two volumes of hydrogen and one of oxygen form water, is it not somewhat paradoxical that such matters should produce such a fluid, when we consider the nature of the gases and the quality of water? For instance, we find that these constituents cannot be compressed into a liquid state. Again, hydrogen is one of the most inflammable substances in nature, and yet water cannot be set on fire. Oxygen again is the pabulum which enables bodies to burn with brilliancy, and yet water extinguishes combustion.

Again we find that, while *three* volumes (altogether) of the gases are necessary to form the water, yet that the water resulting from their combination when brought into a gaseous state or that of steam, *only occupies two volumes;* so that one of these volumes has mysteriously disappeared.† Now this circumstance alone ought to have been enough, I

* Thomson's Syst. Chem. p. 248.

† Does not this prove the decomposition of the hydrogen, which, having lost its imponderable phlogiston, water and oxygen are all that are left behind?

submit, to call upon chemists to reconsider the question, and to examine again and again Cavendish's process. Not so, however—the theory has been so long accepted, and the belief in it is so deeply rooted, that a possible fallacy is never once contemplated; and we consequently find, that the only effect it produces, is to call up the imaginative powers of a most ingenious man to reconcile the anomaly with the theory. This difficulty, then, the scientific and accomplished Dr. Prout thought he was bound to grapple with; and accordingly, in his highly estimable Bridgewater Treatise, he thus accounts for the difference in quantity between the matters producing and the compound produced.

"One volume of oxygen has contributed to form two volumes of water, which two volumes of water must consist *of twice the number of self-repulsive molecules contained in the one volume of oxygen;* yet every one of these molecules must contain oxygen, because oxygen is an essential element of water; it follows therefore *irresistibly*, that every self-repulsive molecule of oxygen has been divided into two, and consequently must have consisted of *at least* two elementary molecules *somehow or other associated, so as to have formed only one self-repulsive molecule.*" And a little farther on Dr. Prout adds, "Now, as we cannot admit the division of an ultimate molecule or atom, we must of course conclude that the molecules of oxygen and hydrogen are much more compounded, and must *each of them* contain *at least* three component or sub-molecules. *Hence the self-*

repulsive molecules of water will consist of *at least nine compound molecules.*"*

This, then, is another theory which we must absolutely accept as a corollary of that on the decomposition of water; a theory upon which I shall offer no other comment, than that it cannot be read without exciting our admiration of the mathematical ingenuity of its author, who in his coerced notions of the atomic constitution of water, has exhibited a fertility of imagination which far outstrips anything, I may venture to say, which is to be met with in the pages of Hermes Trismegistus, Geber, or Raymond Lully, or any other of the mystic writers on the *Lapis Philosophorum.*

It is impossible to say to what extent the mischief of this supposed discovery of the constituents of water has gone: it has vitiated the whole system of chemistry. Even the atomic theory of Dalton is built upon the assumption that hydrogen is an element, and that one atom of that gas and one of oxygen form water; and hydrogen being taken by him as unity, he necessarily makes an atom of oxygen to weigh eight in order that the two gases united may weigh nine, the weight of one atom of water.

But this atomic theory, though generally received, is faulty; and how could it be otherwise, when hydrogen is treated as an element? We consequently find, that notwithstanding Dalton's ultimate atoms are *indivisible,* one of the oxides of lead

* *Vide* Bridgewater Treatise by Dr. Prout, pp. 123 and 126.

(the yellow oxide) is upon this system composed of *one atom and a-half*, which being impossible, a supplementary theory has been suggested, in order to remove the objection, but which being found incompatible with practice, has not, we are told, been adopted.*

I shall now conclude this part of my inquiry, with an appeal to the Noble President and Council of the Royal Society, and respectfully ask that influential body, whether, after deliberately weighing the arguments and evidence upon this important topic, they can think it consistent with the high character and more than European celebrity of the Society, constituted as it is for the especial promotion of science, and bearing on its banners the all-exciting and imperishable name of NEWTON, to remain quiescent spectators, and allow this all-absorbing question to slumber over the next half-century.

Surely it must be the paramount duty, as well as the interest of the Society, to institute a series of experiments under its direction, in order to determine, ONCE and FOR EVER, whether water is or is not a compound body; and if it is, what are the exact proportions of its elements; and further, whether hydrogen is or is not a compound of electricity and water.

I do not entreat the Society to have these experiments made, under any doubt whether hydrogen is

* Dalton, we have seen, chose hydrogen for unity, for the reason, he says, that it is an element which unites in the smallest proportions with others.

or not a compound ; or whether, upon its combustion with half its volume of oxygen, an acid mixture will or not be produced ; as I am satisfied that if any chemist, after reading this pamphlet, will take the trouble to peruse attentively the papers of Cavendish, Priestley, and Watt, referred to in the note,* *he must inevitably* arrive at the *full and entire conviction* of the accuracy of the following conclusions, viz.

1st. That hydrogen is a compound of electricity and water.

2dly. That the addition of atmospheric air, or oxygen, only renders the hydrogen capable of combustion by the application of *additional phlogistic matter*, supplied either by the electric spark, an ignited wire, or a flame, &c.

3dly. That the result of the deflagration of the hydrogen is its decomposition, and the consequent restoration of the water.

4thly. That the water so restored will absorb the oxygen and become acid ; the acidity being in proportion to the *quantity of oxygen* which has been added to the hydrogen.

5thly. That if atmospheric air be substituted for the oxygen, the water will be comparatively tasteless.

My appeal to the Society then, has exclusively for its object the establishing of these facts under its

* Cavendish's paper vol. 15 Phil. Trans. abr. p. 481
Watt's paper . . id. . . . 555
Cavendish's paper vol. 16 . . . 451
Priestley's do. . . id. . . . 518
Priestley s do. . . vol. 17 . . . 55

SANCTION and AUTHORITY, in order to put an end to a theory which has subverted the principles of Chemistry, and rendered futile every chemical analysis since the days of Priestley.*

That this theory should have maintained its ground for so long a period, may be attributable not merely to the cause I have before alluded to, but probably in some measure to the astonishment, or to use a French expression, to the *eblouissement*, which the apparent magnitude of the discovery occasioned, and to the consequent indisposition to call in question a conclusion so flattering to scientific vanity.

Another cause may have been the obscurity which has prevailed with regard to the electric fluid, and our ignorance of its being an element of metals, and of all combustible bodies.

That chemists, however, should have been familiar with the operation of the voltaic pile; that they should have known that a metallic calx will become again a metal on receiving an electric current, and that the metal so revived is lighter than the previous calx;† and yet that they should not have discovered *from what source the galvanic fluid derived its elec-*

* It will be an extraordinary occurrence, if the Royal Society, which first gave publicity and its sanction to the theory of Cavendish, should, after a lapse of *sixty years*, be the first to proclaim it a decided fallacy; and yet, if the solemn admonition, *Dum loquimur fugerit invida ætas*, have its due weight, and the interests of science are alone consulted, this course cannot be long delayed.

† Evidently arising from the oxygen having been driven away by the power of the current to give place to the electricity.

tricity, will certainly be one of the most remarkable circumstances in the history of modern science.

Having disposed of the questions respecting water and hydrogen, I will now proceed to the consideration of the ELECTRIC and MAGNETIC fluids, and their mode of action.

There seems every reason to suppose that the galvanic and magnetic fluids are only modifications of the electric fluid, as all the experiments on these matters go to establish that identity.

The source of the electric fluid cannot now admit of a doubt. It is evident from what has been already stated, that this fluid is one of the elements of metals from which it is liberated on their being partially decomposed or oxidated; and it is equally certain that the quantity of the electricity depends upon the rapidity with which the metals are oxidated.

Davy says, "that atmospheric air or oxygen, or nitrous or muriatic acid in solution in the water, will produce the oxidation of the metals, (*but not pure water alone*), and that the galvanic phenomena are in proportion to the *rapidity* with which the metals (the zinc particularly), are oxidated." This he repeats in another place, remarking, "that the plates will act no longer than the water between them holds *some oxygen** in solution."

* See Dr. Paris's Life of Davy, vol. i. p. 246. How impossible is it then to suppose that oxygen can be one of the elements of water, when it is the absence of oxygen which prevents the action of the water on the metallic plates.

Davy also says, "that the *electric and galvanic fluids are the same*, the apparent difference depending on the intensity and quantity."

I should rather conclude however, that the difference is not merely from the cause Davy states, but that hydrogen and oxygen are associated with the electricity.

Professor Daniel says, that so much hydrogen was evolved from, what he calls, the negative metallic surface of the plates of the voltaic pile, and which he thought consumed (used up) a portion of the electricity to the disadvantage of the working of the battery, that he was induced to alter the arrangement of the apparatus.*

We have also seen from Mr. Sylvester's experiments, tested by Davy, that oxygen is carried with the electric fluid.

In corroboration of my opinion, we find Dr. Clarke to state that two volumes of hydrogen and one of oxygen produce a stronger decomposing force than the most powerful battery,† and which must be attributed to his using a greater portion of *pure oxygen* than associates with the electric fluid in the voltaic pile. The accuracy, however, of these ideas must be tested by future observation.

With regard to the nature of the electric fluid in its simple state, Franklin treated it as having two properties, the one positive and the other negative.

* See 21st vol. Ency. Britan. p. 669.

† See Clarke's Treatise on the Blow-pipe, quoted *antea*.

The French chemists believe that there are two distinct fluids, and we appear now to have adopted this conclusion.*

From the apparent mysterious mode of action of this matter, Davy thought " that the phenomena of electricity, were produced by a highly volatile *fluid* or *fluids* of which the particles are *repulsive with respect to each other, and attractive of the particles of matter !*"

Dr. Prout has in his Bridgewater Treatise given the prevailing opinion with respect to electricity and the cause of its phenomena. " It seems," he says, " to be *satisfactorily proved* that the phenomena of electricity depend upon *two energies* mutually existing throughout nature in a state of equilibrium, in which state their peculiar powers are not perceptible,—that these energies can be *partially separated* and kept asunder."

" That if two bodies charged *in excess with the same energy*, be brought into the vicinity of each other, they mutually repel each other ; whilst two bodies *charged with the two different energies* mutually attract each other."†

* For if there be but one fluid, thought the French chemists, how inconceivable it is that it should possess two opposite properties, one attractive and the other repellant, and that these two properties should be found in the smallest imaginable portion of the fluid, and yet be inseparable. The French philosophers had shaken the belief in miracles.

† See 8th Bridgewater Treatise, p. 36.

Priestley had before observed, that Mr. Symner had produced a number of experiments to shew the existence of *two* electric fluids, *not independent, but always co-existent*, and *counteracting one another.**

Now all these theories, which carry with them an absurdity at which our sense revolts, are erroneous. There are no two fluids; there are no such things as positive and negative electricity; and the supposed attractive and repulsive properties have no existence.

The electric fluid, which there can be little doubt acts an important part in the arrangement of the atoms of all substances,† *is constantly circulating* upon our globe in the direction of the north pole; and it is from this simple fact alone, as I shall presently shew, that flow all the phenomena of electricity and magnetism which have puzzled the greatest men of modern times.

It may be said that this is a mere hypothesis; but it is surely one that resolves itself into a certitude, when we find that electricity in a free state is always in motion, and when the magnetic needle shews us that this motion is invariably directed to the north pole; and this assurance becomes still greater, when we find that it accounts for all the phenomena of electricity.

But all doubt upon this point vanishes the mo-

* Priestley's Hist. of Electricity.

† Davy says—"I have shewn that chemical attractions may be *exalted, modified, and destroyed by changes in the electric state of bodies.*"

ment we find that if the needle be so magnetised as to destroy the current, the *polarity does not exist.**

Every substance in nature seems charged with more or less of the electric fluid, according to its constitution;† and all excess of this fluid existing on any matter whatever, passes from the matter into the general current; and this passage of the fluid necessarily takes place with more or less facility according as the body it is upon, is more or less a conductor of electricity.

"Common electricity," says Davy, "is *excited* (I should add, "and accumulated") *upon non*-conductors, and is readily carried off by conductors and imperfect conductors."

Now, there are three sorts of substances, viz. conductors, *par excellence*, imperfect conductors, and non-conductors.

To make myself perfectly intelligible, I will beg to state, that if we want to obtain any part of the electric fluid in circulation, for any purpose, we use a non-conducting body, which being excited in the ordinary way, is found to be *charged* with an accumulation of the fluid, which is then, by a conductor, passed on to the Leyden jar, or any other apparatus for the deposit of the fluid; such jar or apparatus having been previously placed upon a stand furnished with glass or non-conducting feet, so that the fluid can have no escape from it,

* See page 54.

† The late Mr. Becquerel, of the French Institute, says "*it is anincontestable fact*, that all bodies contain between their molecules a neutral electric fluid." See Ency. Britan. vol. 8. p. 599.

except by the atmosphere, which when in a dry state is also a bad conductor.

Now if the electric fluid be perpetually circulating towards the north, it is evident that every portion of such fluid, and any modification of it, (such as the magnetic fluid) which may have been so collected, must have a tendency to join and be carried with the fluid so in circulation. These simple data being premised, the consequences are self-evident.

For instance, suppose a rod of metal about fifteen inches long, and three quarters of an inch in diameter,* *a*————*b*, to be charged with the common electric fluid, by introducing such fluid at the end, *b*. Now metal being an excellent conductor, it is clear that this fluid in order to pass with the general circulating fluid, will proceed instanter upon the rod from *b*, to the other end, *a*; and it is equally clear, that as the atmosphere is not nearly so rapid a conductor as the metal, the fluid, when it reaches *a*, must remain accumulated there, until by the slow conducting power of the air it is carried off to join the general current.

We will now come at once to the cause of the polarity of the magnetic needle, a problem which might have remained unsolved for ages, from the strong disposition of the mind for what is mysterious —the "omne ignotum pro magnifico."†

* Of course a rod of any other dimensions would equally answer the purpose.

† It is a remarkable circumstance that the predilection for the marvellous is to be found in all grades of society.

The ablest mathematicians, and the closest logicians are tinc-

Let us take then a small steel bar, similar to what we see in a Dolland's pocket mariner's compass

tured with it as well as the uninstructed. About two years since I heard the celebrated French Astronomer and perpetual Secretary of the Institute, Mr. Arago, say,* in speaking of our sight, that we saw objects double, and those in an inverted position, defects which we only corrected by experience, or as he forcibly expressed it, by education. This doctrine, he told us, was founded upon experiments made by Chesselden and others upon persons born blind, and who, when they had acquired their sight, had their vision thus imperfect.

Now, instead of this strange and absurd doctrine (which I believe is not exclusively Mr. Arago's, and is to be found in the books,) what does *common sense* say upon the subject? This, that the optic nerves of these patients from long want of use, could not perform instanter their functional duties, and that time would be required to enable them to recover their primitive power.

Will any man say that this is not a clear and natural, and therefore a satisfactory reason? Why then resort unnecessarily to the marvellous?

We know that objects are inverted upon the retina; but does it follow, as an inevitable consequence, that the impression upon the mind is also inverted? Then why gratuitously assert as a fact that which is contradicted from our earliest experience?

This subject I more fully entered into, and illustrated in a paper, which appeared in the "Lancet" some months since, and which contained an experiment that removed all doubt upon the subject.†

I had sent the same paper some time previously to Sir David Brewster's "Philosophical Magazine" for insertion, but the Doctor thought proper to reject it, so indisposed are these great Professors and living Depositaries of "useful knowledge," to be put right upon any subject on which they are mistaken, and have been for years misleading the public.

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* In Paris, at a Lecture on Astronomy.

† See "Lancet," 21st June, 1845, p. 719.

a——————*b*. Now, if we wish to magnetize this bar, so that the north pole shall be at the end *a*, we must pass the magnet along the bar from *b* to *a*, and always repeat the operation in *that* direction. If on the contrary we desire the north pole to be at *b*, we must pass the magnet the reverse way, or from *a* to *b*.

It is evident that in either way we produce a magnetic current, and therefore from whichever end this current escapes, that end (when the bar is suspended, so as to turn freely as in the compass), must point to the North Pole, such being the direction the fluid is travelling.

That this is the case will, I think, be indisputable, when we look at the effect of opposing current to current, as in the following experiment.

Let the bar of steel *a*——————*b* receive the magnetic fluid in the direction *b*—*a*, that is by passing the magnet several times from *b* to *a*. Instead of now continuing the operation in the same direction, let the magnet be passed several times on the same bar from *a* to *b*. What follows? There will be now no polarity at either end of the bar, when it is suspended as before; and, therefore, for the purpose of indicating the North Pole, the bar will be useless. Why is this? Because, as must be manifest, by thus reversing the currents we oppose the current *a* to the current *b*, and which two currents must consequently meet in some *intermediate* part of the bar, where an accumulation of fluid will be formed, and where the fluid must escape

in the best way it can; for as neither current can return upon itself, so neither current can reach either extremity of the bar.

In this operation, therefore, the magnetic fluid will appear to be neutralized and its properties annihilated; an evident proof that without a current there can be no polarity.

With regard to the phenomenon called the "Dipping of the Needle"—this no doubt arises from some inequality on the earth's surface, by which the fluid takes a downward course, and in that direction carries the needle. Or there may be large cavities near the surface containing metallic strata through which the fluid passes on its journey round the earth.

It will be hardly necessary to illustrate this magnetic theory farther, as the other phenomena will be explained by those resulting from electricity; but I will just notice the horse-shoe magnet, lest it should create a difficulty in the mind of the reader.

In this magnet, then, $_{a}\overset{b}{\cap}_{c}$ two currents exist, the steel in this form being magnetised by passing the magnet communicating the fluid, first from *b* to *a*, and then from *b* to *c*.

It is evident, in this case, that the currents do not oppose each other, but take contrary directions, and consequently the strength of the magnet consists in having a double quantity of fluid, by a current terminating at each extremity.*

* It will be also remarked that *unlike* a magnetised bar, one end of which is always positive, and the other negative, both

The power called attraction is evidently nothing more than the tenacity with which the particles of the fluid adhere together, and in that state pass from conductor to conductor, thus causing an apparent cementing together of the two conductors.

The tenacity of the magnetic fluid must be owing to the combination of the electric fluid with some matter at present unknown.

We will now proceed to examine the supposed positive and negative properties of the common electric fluid, and in which the adhesion of the particles is not so great as in the magnetic fluid.

Suppose, then, two rods of brass, about fifteen inches long, and three quarters of an inch in diameter, 

and that each rod is charged with the common electric fluid, introduced at *b*.* It is evident, that if these two rods, so charged, be kept isolated, the fluid will have accumulated at the end *a*, of each rod. Now, then, remark what follows. If I place the end *a*, of rod 2, in contact with the end *b* of the other rod, I shall form, as it were, but one conductor; and it is therefore evident, that the fluid which

the extremities of the horse-shoe magnet are positive, and which (as will be readily understood) arises from the manner of magnetising the latter magnet.

* In all these experiments it is evident that the glass, or non-conducting handle, must be so attached to the middle of the rods, that the entire rods with their ends or extremities may be perfectly free.

exists on the rod No. 2, will quit that rod, and pass on to join the fluid of rod No. 1, at *a* ; and if I charge a thousand rods, and place them together under the same circumstances, so as to form but one conductor, all the fluid will pass on to the first rod, on its way to join the general current.

Again, if I place the ends *b* of the two rods so charged together, no phenomena will take place, it being evident that in this position of the rods there can be neither attraction nor repulsion, nor any other apparent property manifested, as the fluids are now travelling in different directions, and wholly independent of each other.

But suppose, instead of placing the end *a* of one rod to the end *b* of the other, (which produced the phenomenon of attraction), we oppose the ends *a* of the two rods, that is, place them nearly together, what must be the manifest result? It is clear that the two accumulated fluids will be now placed in a state of antagonism, and that, like two currents of water, or any other two fluids in motion, opposed to each other, instead of attraction, a violent commotion will take place, and that all matters found or placed within their sphere of action must be forcibly carried away. Now, this antagonism taking place, with the magnetic or the common electric fluid, produces the phenomenon called repulsion.

Thus, then, is explained all the mystery of attraction and repulsion, and of positive and negative properties, about which volumes have been written.

If, however, all the phenomena of electricity had been confined to these peculiarities, it is to be hoped, for the honour of science, and the acumen of its professors, that the occult nature of these operations would have been sooner brought to light.

It, however, happens that substances are met with upon which are found *only* positive electricity, as it is called; and upon others *only* negative electricity. Thus, then, it should appear as if there must be two fluids, as they are now apparently found detached.

Then, again, some substances will receive electricity from some bodies and refuse it from others, so that it is concluded that certain bodies will only receive a peculiar kind of electricity.

Now, this is all illusory, as will be readily understood and admitted upon examining a little farther into the nature of the different conducting bodies.

The common electric fluid is excited and collected (as we have seen) upon a body that does not conduct—for instance glass. Why? because the fluid must remain on such body in a comparatively motionless state—an uneasy state, to use a metaphor—a desire to leave but an incapacity to do so—a wish to arrive at the upper story of the building, but can find no staircase which leads to it. Suppose, then, the fluid to be now existing on such a surface.

Suppose again a *conducting* body to be *charged* with the electric fluid—(one of the metallic rods for instance), which has received such fluid at *b*, and

which has consequently passed on to *a*. Now, place the end *a*, of this rod (the positive end as it is called), where the fluid has concentrated itself, in contact with the fluid which is stagnant on the non-conducting body. It is plain that in this case there can be no attraction—why? Because both the fluids are endeavouring to escape, and in this situation neither of them offers to the other any facility or means of so doing; on the contrary, the result of their approximation is to produce a scuffle between them to the advantage of neither. But instead of the positive end, *a*, place the negative end,* *b*, of this conductor on the non-conducting body, and you immediately discharge the latter of its electricity.

It is evident, that as the fluid on glass or any other non-conductor is struggling (as it were) to escape, such escape will be rendered more difficult by opposing to it the positive end of a conductor *charged* with other fluid; but that if the negative or *uncharged* end of the same conductor be offered to the fluid on the non-conducting body, the latter fluid will immediately attach itself to this uncharged end.

Now, to account for this result, instead of the above simple explanation, our scientific men have created two fluids, calling that upon the metallic rod the positive, and that upon the non-conducting body the negative fluid; and they have assumed from the above experiments that there exists a warm attachment (affinity) between the positive and negative families, while on the contrary these two

* That is the end which the fluid has left.

mysterious fluids, as between themselves, that is when in their separate establishments and confined to their respective houses, are upon terms of utter enmity, and that nothing but discord reigns between them.

It is found again that the electric fluid upon two non-conducting substances will not unite; that when one non-conducting substance, charged with the electric fluid, is presented to another non-conducting body also charged, no sympathy takes place between them, but quite the contrary. I have already given the reason of this, namely, that both fluids are in an imprisoned state, their motion in a current being suspended; and as each fluid is seeking (if I may be at liberty to use this expression) to escape, it is evidently not the opposition or the union of the two fluids which can effect the object, but a conducting body, which neither of them possesses; and consequently placing the two non-conducting substances together only embarrasses their condition. The result then is, that a juxta-position of two bodies under these circumstances produces no other phenomenon than repulsion.

Now, there occurred a circumstance which enveloped this occult subject, (as it appeared to the great practitioners of electricity), in impenetrable obscurity, and called in vain for professional ingenuity to account for.

Dr. Faraday found that a metallic ball isolated—that is, suspended in the open air by a silken

string, would receive or be charged with the electric fluid (by induction, as it is called), from the atmosphere.

This, according to what I have laid down, is an inevitable consequence. The fluid being always in circulation, and metal being a powerful conducting surface, the ball must in this case be charged with electricity.

But the surprise of Dr. Faraday arose from his finding, that notwithstanding the ball was of metal, and therefore the best of conductors, it was charged with the *negative* fluid; a discovery which must have caused him considerable astonishment.*

Now, this again, according to the theory I have stated, is not only perfectly intelligible, but is also a necessary consequence; whereas, according to the theory of the books, it can in no shape be accounted for, and must have been in direct opposition to all Doctor Faraday's ideas on the subject.

To explain this apparent anomaly, I have already stated that a body is a non-conductor, or an extremely bad conductor, when the fluid rests upon it in a comparatively torpid or imprisoned state, the natural condition of the fluid being that of a current, which in this case of the ball, cannot exist. I must here observe, that to constitute a good conductor, it is not exclusively a metallic substance which makes it so, but also the *shape* of the metal, and which must be such as to

* See Dr. Faraday's paper in Sir David Brewster's Philos. Mag. of June, 1843, p. 478.

facilitate the transit or passage of the electric fluid. It is for this reason that metallic *rods* are employed; it being evident that if the metal has either a circular or an oval shape the fluid will be dispersed over the entire plate of metal, which must be altogether destructive to the formation of a current. The more the positive end of the rod is pointed, the easier the fluid will necessarily pass.

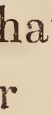
It is evident therefore, that although metal is an excellent conductor, it may be so shaped as to have all the disadvantage of a non-conductor; and in that case, all the difference that can exist between a metal and a non-conducting body will be, that the metal will be highly charged with the electric fluid by induction (that is, by exposure to the atmosphere), whilst a non-conducting body will receive little or no electricity,

Now, when the metal is formed into a ball, it is precisely of the shape in which it must act as a non-conducting surface or body.

How can the electric fluid pass off from the metal in this globular shape, better than it could from any non-conducting surface? But the moment this same ball of metal is by the hammer elongated, and offers an extremity or end for the fluid to escape from, the metal will then be no longer negatively but positively charged by induction or exposure to the atmosphere.

It is clear from what has been stated that a ball of metal *cannot* be charged with positive electricity by any means; neither by induction nor by the electri-

cal machine. Its form places it in the condition of a non-conducting substance, and no non-conducting matter can be charged with the so-called positive electricity. Positive electricity, be it remarked, is the fluid at liberty,*—viz., in motion as a current,—and which it cannot be on a non-conducting surface, or on a metal of a form not allowing of a current.

However, to put this subject beyond the possibility of cavil, and to prove, incontrovertibly, that the ball is charged with the supposed negative fluid, solely from the cause I have mentioned, if, when the ball is so charged, a short metallic rod  be placed upon, or attached to the ball, with the upper end *a* pointed, so as to facilitate the passage of the fluid into the atmosphere, the fluid on the ball will be found immediately changed from a negative to a positive fluid; and it will be also found, that so long as the pointed rod remains affixed to the ball, it will be impossible to charge the latter, by induction or otherwise, with negative fluid.

It is evident, that by attaching a rod of metal to the ball, a current will be aided; and it will be found that from that moment the fluid will resume its natural motion, and become, to all intents and purposes, according to the nomenclature of the day, a positive fluid.

I will now say a few words respecting the *modus operandi* of the voltaic pile.

* Negative electricity is the same fluid, not moving in a current.

All bodies appear to be in different states of electricity,—i. e., to have more or less of the fluid, according to their respective natures; and this fluid must consequently be considered an essential agent in the constitution of all compound substances.*

When the active power of this fluid is made more energetic by accumulation and intensity, or by combination with some other matter, by which it becomes what is called the galvanic fluid, it has the virtue of disintegrating or separating certain compound substances; and which it most likely effects in as simple a manner as the crystals of sugar are dissolved in water, and from a similar cause, viz. the *superfluity* of an element which destroys the arrangement of the matters of which the body is composed.

The construction of the voltaic pile is such as to compel this fluid to travel in a circle, and which is effected by employing metals of different conducting powers, so that the fluid quits in succession an inferior conducting power for a superior one,—viz. zinc for the copper. By this means the whole of the fluid is made to circulate, and consequently to act upon any given substance so placed as to intercept the current.

When the resolution of a compound body takes place, the substances of which it is compounded will necessarily arrange themselves according to the order of their electric capacities, (which can be

*. See Davy's remark in note, p. 50.

readily imagined from what has been already written) that is, the body which has as great or a greater conducting power than the substance forming the positive pole will pass to that side, and the bodies having a less conducting power than the substance forming the (so called) negative pole will attach themselves to the latter, and thus extend the conductor of the fluid according to their respective electric capacities.

Here, then, we again find a total absence of all that mystery in which philosophers so fondly indulge.

The highly romantic notions which have up to this time been entertained of the properties of matter, are, I am inclined to think, to be attributed in some measure to scientific vanity. The learned Professors and Doctors in Philosophy never could stoop to believe that the Secrets of Nature could have been intended to be revealed to those humble individuals who merely possess ordinary common sense, and the consequence has been, to exaggerate the works of nature to dimensions which they concluded could only be grasped by master minds.

As, however, it now appears that we can dispense with double electric fluids, "self-repulsive molecules," and other miraculous atoms of scientific creation, and that the Supreme Intelligence has not thought proper to form the materials of this world for the exclusive comprehension of those wise men, who, like the dogmatic critic in the "Athenæum,"*

* See note, p. 15.

assume a monopoly in science, it may be expected that in any future interpretation of Nature's works, they will condescend to make simplicity and not complexity their guide.

For with regard to the electric matter, we may use the words of Sir Kenelm Digby upon another occasion, and say "that the simplest person that can but apprehend and speak sense, is as much a judge of it as the greatest *Doctor* in the school."

To conclude, we find then, that all the phenomena we meet with in electricity and magnetism (except light and heat, the inherent properties of electricity*), flow from the simple circumstance *of the continuous circulation of these fluids in a northerly direction.*

However strange and incredible (considering the variety of the phenomena) this may appear, and however it may call forth the opposition of those philosophers whose minds repugn so humble a theory, I must continue, like Galileo,† to exclaim, laconically, but emphatically, It is nevertheless so!

* Mr. Morgan has proved that there is no fluid or solid that may not be rendered luminous by the transmission of an electrical discharge through its substance.

† "*E pur se muove!*"

ADDENDA.

The author's Theory of the Phenomena of the Electric and Magnetic Fluids, and their mode of action, suggested itself to his mind during an illness at Aix-les-bains in Savoie, and was thence communicated by him to the "Royal Society," in a paper transmitted to their excellent Secretary, Dr. Roget, in August, 1834, and subsequently to Dr. Faraday, in January, 1838.

The subject was referred by the Society to a Committee, but no report was ever made.

In December, 1834, the same theory was communicated by the author to the "French Institute," in a memoir presented by M. Arago, when the subject was referred to Messieurs Ampère and Becquerel, but they never made any report.

The author also communicated the Theory of the Magnetic-Needle to the "Board of Admiralty," in a paper transmitted in February, 1838.

Early in 1844, in consequence of seeing in the Philosophical Magazine of Sir David Brewster's, for June, 1843, Dr. Faraday's experiment noticed in the foregoing pages, the author addressed another memoir to the "Royal Society," accounting for the phenomenon, and shewing that it flowed from, and was confirmatory of, his Theory.

The same communication was also sent, in March, 1844, to Sir David Brewster's Philosophical Magazine; but it was never published, as the author believes.

The Theory as to the Non-Decomposition of Water, and that Hydrogen was composed of Electricity and Water, was communicated to the Royal Society in a memoir forwarded from the continent to Dr. Roget, in April, 1840, and which was referred by the Council to the "Committee of Chemistry," who have made no report thereon.

A similar paper was sent to the Marquis of Northampton, in May, 1840.

In December, 1841, the author addressed another memoir upon the same subject, to the Royal Society, accompanied with the dicta of Cavendish, Priestley, and Watt, as to Hydrogen being a compound body, which was also referred to the "Committee of Chemistry," but without any result.

THE END.

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